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THE WAGE EFFECTS OF SEXUAL ORIENTATION DISCRIMINATION

M. V. LEE BADGETT*

This study is the first to apply the econometric tools developed in the study of race and gender discrimination to the newer question of sexual orientation discrimination. Analyzing pooled 1989–91 data from a national random sample, the General Social Survey, the author finds that gay and bisexual male workers earned from 11% to 27% less than heterosexual male workers with the same experience, education, occupation, marital status, and region of residence. There is also evidence that lesbian and bisexual women earned less than heterosexual women, but this result is not consistently statistically significant across all variable definitions and specifications.

Over the past three decades, legislators at the federal, state, and municipal levels have moved toward a general public policy stating that employers should employ and pay people based on what they can produce on the job, not who they are. The Civil Rights Act of 1964 prohibits employment discrimination because of an individual's race, color, religion, sex, or national origin. Other laws forbid employment discrimination based on individuals' age or physical or mental disability. Other salient aspects of human identity remain outside this form of legal protection, however. Most notably, employment discrimination against individuals who are or are

perceived to be lesbian, gay, or bisexual remains legal in most workplaces in the United States. This paper is the first econometric study of the possible wage effects of such discrimination.

Much of the debate about adding sexual orientation to civil rights laws has centered on the need for such legislation. Proponents of civil rights protections for lesbian, gay, and bisexual people argue that these people experience employment discrimination and that it causes them economic and psychological harm. Opponents of civil rights protections argue that such laws are unnecessary and would grant gay people "special privileges" (Carroll 1992). Citing survey data purportedly showing that lesbian and gay people have higher than average incomes (see, for example, "Clinton Administration Backs Bill," 1994), they assert that lesbians and gay men are an affluent group without need of further protection, and call into question the very existence of discrimination against them.

Existing economic studies of lesbian and gay people, however, are based on biased samples and inappropriate statistical com-

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parisons. In this paper I apply econometric tools developed in the study of race and gender discrimination to the newer question of employment discrimination based on sexual orientation. Data pooled from the 1989–91 versions of the General Social Survey, a national random sample, allow econometric testing for the effects of sexual orientation discrimination on earnings. In its use of a random sample and multivariate analysis, this study constitutes a significant methodological advance over other quantitative studies of such discrimination and past efforts to compare earnings by sexual orientation.

Conceptual Framework

Before evaluating labor market outcomes for evidence of employment discrimination against a group, it is reasonable to ask whether there is some reason to expect differential treatment of that group. Historical, sociological, and psychological research demonstrates the existence of homophobia (the fear of homosexuals and homosexuality) and heterosexism (the belief that heterosexuality is superior to homosexuality and should be an enforceable social norm) and the effects that such attitudes have in the everyday experience of lesbians and gay men: the lack of social or legal recognition of family structures, the persistence of threatened and actual violence, and the perpetuation of false stereotypes (see, in general, Gonsiorek and Weinrich 1991). Individuals with a bisexual orientation may also encounter such attitudes, although at other times those individuals may be perceived as being (or at least behaving as) heterosexual (see Garnets and Kimmel 1991:149).

If employers or coworkers have a distaste for gay identity, behavior, or “lifestyle,” employers may develop a taste for discrimination (following Becker 1971). This taste for discrimination is necessary but not sufficient for the occurrence of discrimination against gay employees, however. Unlike race or gender, for instance, sexual orientation is not generally an observable characteristic. (In this respect, sexual ori-

entation is more like religion or national origin.) For the social stigma attached to homosexuality and bisexuality to result in direct employment discrimination, *disclosure* of a gay employee’s sexual orientation is necessary.

Lesbians and gay men who voluntarily disclose their sexual orientation to employers or coworkers may trade off the risk of diminished career advancement or income loss for some future return (for anecdotal examples, see Woods 1992:216–22). The future return may be psychological (enhanced self-esteem), political (a more supportive and accepting workplace), or economic (extension of benefits to domestic partners or spousal equivalents). Given these potential trade-offs, disclosure is likely to be endogenous, that is, at least partly determined by workplace factors.

An important workplace factor, of course, is income, but the direction of influence is difficult to determine *a priori*. First, compared to lower-income workers, higher-income workers may occupy jobs that allow them to manage the harmful effects of social stigma more comfortably, increasing their probability of disclosure. Second, higher levels of income could cushion the financial burden of some adverse workplace reactions, such as a loss of promotion, again leading to more disclosure by high-income people. In the other direction, however, people earning high incomes would lose more if fired, for instance, particularly if they feared that information about their sexual orientation would hinder their job search.¹ Schneider (1986:479) found that lesbians with higher incomes were less likely than those with lower incomes to disclose their sexual orientation; that study, however, used a nonrandom sample.

¹Such a firing occurred in the case of Jeffrey Collins, a former Shell Oil employee. A California court found that Shell had fired Collins for being homosexual, violating the implicit employment agreement between Shell and Collins. Furthermore, Shell informed corporate headhunters of the reason for Collins’s dismissal (*Business Week*, 1991).

Disclosure of sexual orientation may also be involuntary, although the extent of this kind of disclosure is unknown. Inferences of a lesbian or gay identity can be made by employers or coworkers from numerous sources of information: military discharge records, arrests or convictions, marital status, residential neighborhood, silences in conversations, and so on. In some cases, these inferences may even be incorrect, and a heterosexual employee may be wrongly perceived as being gay. Furthermore, voluntary disclosures to coworkers increase the likelihood of involuntary disclosure, whether accidental or deliberate.

Disclosure, whether voluntary or involuntary, may result in sanctions by coworkers, supervisors, or employers. Negative reactions will vary across workplaces. Coworkers might harass a gay or lesbian coworker, with adverse effects on the worker's productivity, income, and advancement. Supervisors or employers may harass, fire, or refuse to promote lesbian, gay, or bisexual employees.² This form of *direct discrimination* results in equally productive individuals being compensated differently.

Although disclosure is necessary for direct discrimination to occur, even successfully passing as heterosexual might not preclude negative economic effects. Evidence suggests that nondisclosure (or "passing" as heterosexual) is a common strategy for avoiding discrimination (Badgett, Donnelly, and Kibbe 1992). As Escoffier (1975) pointed out, passing may require a conscious effort to avoid potentially awkward social interactions that contribute to job satisfaction or advancement for other workers. The isolation involved in many passing strategies could lead to higher absenteeism and job turnover, and the energy devoted to passing might reduce productivity. In this case, the behavior is not an intrinsic characteristic of the worker but an effect of *indirect discrimination* within a workplace perceived as threatening. Two indi-

viduals with equal productive abilities would have differential productivity and, therefore, differential wages because of the work environment's effect on the gay individual's productivity. A different passing strategy could lead to a positive effect on productivity if gay workers, "driven by a half-conscious belief that if they just show themselves productive enough, worthy enough, good enough, they will overcome the invisible stigma," become workaholics (Mohr 1988:149).

Overall, the likely wage effect of successfully passing as heterosexual in the workplace is ambiguous. In theory, the effects of direct discrimination in wages could be distinguished from the indirect effects by controlling for individuals' productivity differences. In practice, however, this distinction will be difficult to make, as will be discussed below.

Evidence on the Economic Status of Lesbians and Gay Men

Legal cases provide well-documented evidence of particular instances of employment discrimination against lesbians and gay men. Further evidence of employers' attitudes comes from a 1987–88 survey of employers in Anchorage, Alaska. Of the 191 employers surveyed, 18% said they would fire homosexuals, 27% said they would not hire them, and 26% said they would not promote them (Brause 1989). Some evidence that attitudes do translate into discrimination comes from non-random surveys of self-identified lesbian, gay, and bisexual people. A recent review of 21 surveys found that between 16% and 46% of survey respondents reported having experienced some form of discrimination in employment (in hiring, promotion, firing, or harassment) (Badgett, Donnelly, and Kibbe 1992).³

²Schneider (1986) showed that negative reactions reduce the probability that a lesbian will repeat the disclosure of her sexual orientation in the future.

³These perceptions of discrimination are similar in magnitude to those of African American workers, 36% of whom reported experiences of racial discrimination in a 1990 survey (Turner, Fix, and Struyk 1991:7).

Table 1. Survey Data on Income of Lesbian, Gay, and Bisexual People.

Survey	Year	Instrument	N	Gender	L/G/B Income	National Income ^a
<i>Out/Look</i>	1988	Magazine survey, mail-in	510	Men Women	25K–29K ^b 20K–24K ^b	27,342 18,823
Simmons Mkt. Research Bureau	1988	Gay newspaper inserts			36,900 ^c	(see above)
Teichner/ <i>San Francisco Examiner</i>	1989	Phone survey, random digit dialing	400	Men Women	29,129 ^b 26,331 ^b	28,605 19,643

^a“National income” is median income for full-time, full-year workers.

^bMedian income for sample.

^cAverage income for sample.

Sources: *Out/Look* 1988; Gravois 1991; Teichner 1989. National medians from *Economic Report of the President*, Feb. 1990 and Feb. 1991.

If discrimination does commonly occur and results in similarly qualified and productive people being treated differently only because of their sexual orientation, an economist might expect to observe differences in wages. The paucity of available data for large numbers of lesbians and gay men has made comparisons of income by sexual orientation difficult. Table 1 presents data from three relatively recent national surveys of lesbians and gay men. None of these surveys included heterosexual people, so the comparison group in the last column of Table 1 is the national median income for full-time male or female workers, most of whom will be heterosexual.

The first survey, conducted in 1988 and reported in *Out/Look* magazine, asked respondents to indicate in which \$5,000 range their income fell. The median range reported for gay men, \$25,000 to \$29,999, overlapped the national male median, but the median range for lesbians was higher than the median for women nationally. This same pattern showed up in the 1989 Teichner survey for the *San Francisco Examiner*, which found no clear difference between gay men's incomes and the national male median but higher median incomes for lesbians than for women nation-wide. The 1988 Simmons Market Research Bureau survey appears second in the table and shows the most dramatic differences between gay and national incomes, with gay individuals (86% of whom were men) earning 35% more than the median male.

These comparisons are questionable for several reasons. In the two non-random surveys (*Out/Look* and Simmons), lesbian and gay respondents tended to be disproportionately white, urban, and well-educated, all of which are factors associated with higher average incomes. (The difficulties in collecting data on a representative sample of a stigmatized population are discussed in the next section.) The extraordinarily high incomes found in the Simmons study most likely reflect sample selection bias. Surveys were inserted into eight gay/lesbian newspapers, and responses were mailed in.⁴ This sampling technique guaranteed a highly educated sample (59.6% of respondents were college graduates, 16.8% had a master's degree, and 6.8% had a Ph.D.) and, therefore, higher than average incomes.

But even the survey with the least biased sampling technique (Teichner used random digit dialing and interviewed self-identified lesbian/gay/bisexual people) found that lesbians' median income was well above the median female income. Although the willingness of respondents to identify themselves as gay or lesbian to an unknown interviewer may vary along income lines

⁴Michael Gravois of the Rivendell Marketing Co. provided me with information on the survey in a phone conversation on November 7, 1991. Some results appeared in an article in the *Wall Street Journal* (Rigdon 1991).

within gender groups, which could be a source of observed differences, some further explanation is necessary. Furthermore, without controlling for experience or education, comparisons of the median or average income for the gay sample with the national medians may be misleading. As in the standard economic approach to race and gender discrimination, we need a multivariate analysis of a random sample to properly compare the earnings of lesbian, gay, or bisexual workers to the earnings of heterosexual workers.

Data

As mentioned above, one reason we know little about the economic effects of sexual orientation is that reliable and representative data matching sexual orientation to economic outcomes are extremely rare. One important nationally representative survey, the General Social Survey (GSS) conducted by the National Opinion Research Center, has collected information on labor market variables (employment status, income, and occupation) and, beginning in 1989, on sexual behavior with partners of either sex (Davis and Smith 1991).⁵ While the GSS does not specifically ask about sexual orientation or identity, same-sex sexual experiences are likely to be highly correlated with a self-identified gay or bisexual orientation (see Lever et al. 1992).

Because of the design of the GSS, not all respondents were asked all questions in each survey. Following elimination of those without information on sex partners⁶ or income⁷ (and a few with missing data on

other variables), the sample pooled from the 4426 respondents in the 1989–91 surveys contains 1680 people who were employed full-time when surveyed.⁸ Of this subsample, 4.8% reported having had at least one same-sex sexual partner since the age of 18, a proportion that falls well within the range found by studies of sexual orientation (Gonsiorek and Weinrich 1991). For purposes of this paper, those respondents are classified as *behaviorally* lesbian, gay, or bisexual.

The lack of data on self-identified sexual orientation is disappointing, but using behavior to infer identity may not be inappropriate. First, society may not make such fine distinctions.⁹ Both behavior and identity are stigmatized and are sufficient to trigger legal sanctions in the United States. For instance, the *act* of sodomy is still prohibited in about half of the states (*Harvard Law Review* editors, p. 9), as is the solicitation of “noncommercial, consensual same-sex sexual activity” in many places (p. 27). In some cases, these laws (and the presumption that lesbians and gay men violate the sodomy laws) have been used to justify employment discrimination (see the discussion of *Childers v. Dallas Police Department* in Rubenstein 1993:334).

Second, although respondents are not asked when their sexual activity took place, both absolute and relative numbers of same-sex sexual partners are likely to indicate whether this behavior was temporary or experimental or whether the behavior reflects the individual’s underlying sexual identity and orientation. Therefore, this study uses four measures of gay sexual behavior: (1) having had *one or more* same-sex

⁵More specifically, respondents were asked two questions: “Now thinking about the time since your 18th birthday (including the past 12 months) how many male [female, in the second question] partners have you had sex with?”

⁶The procedures described below were repeated on the sample of all full-time workers, including those without data on sexual partners, who were identified by a dummy variable. This procedure resulted in coefficients virtually identical to those produced by the procedures described in the next section.

⁷Respondents were asked about income earned in the previous year from their current occupation. Thus nonrespondents to the income question are

those who earned no income last year from their current occupation, suggesting that those reporting incomes are strongly attached to the labor force.

⁸Individuals were included if employed full-time when surveyed or if they normally worked full-time but were not at work because of a temporary illness, a vacation, or a strike.

⁹The distinction between behavior and identity is important in some cultures, however, including cultures that are represented in large ethnic groups in the United States, such as Latino cultures (Alonso and Koreck 1989).

sexual partners (the most general definition, identifying 4.8% of the sample); (2) having had *more than one* same-sex sexual partner (3.0%); (3) having had *at least as many* same-sex sexual partners as opposite-sex sexual partners (3.0%); and (4) having had either more than one same-sex sexual partner or at least as many same-sex sexual partners as opposite-sex sexual partners (that is, either (2) or (3), a definition that takes in 3.8% of the sample).

An issue related to the use of this data is their reliability. The questions on sexual behavior in the GSS were self-administered and were accompanied by assurances of confidentiality and by the explanation that "frank and honest" responses were important for understanding how to deal with the AIDS epidemic.

One final drawback of the GSS data set is that respondents were asked to select one of 20 categories to indicate their pretax employment earnings from the previous year, rather than to name exact figures. The income ranges for each category increase with income, but the analysis uses natural logarithms, which evens the ranges out. Medians were calculated for the GSS ranges using Current Population Survey data on full-time workers.¹⁰ All medians were converted to 1988 dollars for the purposes of calculations in this paper.

Methodology

The most common econometric approach for capturing the effects of discrimination is to see if people who are similar in all observable and economically relevant ways have similar labor market outcomes. This paper uses a basic OLS model of wage

determination with the log of income as the dependent variable. Separate equations for men and women will take into account any differences in men's and women's labor market decisions and experiences. Independent variables include individual characteristics related to productivity (such as education, occupation, marital status, and experience)¹¹ and other labor market influences (region, SMSA residence, and race). The main effect of discrimination, if any, will be captured by the coefficient on a dummy variable for being behaviorally lesbian, gay, or bisexual. A statistically significant negative coefficient would imply discrimination in the form of lower wages. Discrimination may also exist in the process of allocating individuals among occupations.

A variable measuring the extent of workplace disclosure of gay behavior or identity would be more appropriate to include in the wage equation, since disclosure is necessary for direct discrimination to occur. Unfortunately, this information is not available. As a result, the hypothesized discrimination characteristic is measured with error, violating one of the usual OLS assumptions. With no information on disclosure, statistical correction is not feasible, but the interpretation of the results can be adjusted.

Overall, this selection bias problem is likely to reduce any measured effect of discrimination, biasing the procedure against the discrimination hypothesis. Consider Figure 1, a matrix of disclosure decisions for behaviorally lesbian, gay, or bisexual people. The framework described above suggests that gay employees disclosing their sexual orientation, the people in groups A and C, are vulnerable to direct workplace discrimination. The variable created from the General Social Survey combines people in groups A and B but does not (and cannot) distinguish people in those two groups, although we would

¹⁰The CPS medians were calculated for all workers, with no differentiation by race or gender. This procedure would tend to narrow the race and gender differences in median earnings in the GSS, since the distribution of earnings *within* a particular range is likely to vary by race and gender. Since this study disaggregates the GSS analysis by gender, observed racial earnings differences in the GSS are likely to be more affected. This probably explains the relatively small OLS coefficients and low t-statistics on the dummy variable for being white seen in Table 3.

¹¹Since actual work experience is not available, potential experience is calculated as (age - years of education - 5).

Figure 1. Voluntary Disclosure Decisions of Lesbian, Gay, and Bisexual Workers.

	Disclosure in Workplace	No Disclosure in Workplace
Disclosure in Survey	A	B
No Disclosure in Survey	C	D

only expect those in group A to face direct discrimination. Lesbian, gay, and bisexual individuals in group B who have not disclosed their sexual orientation at work might still face indirect discrimination if expectations of discrimination reduce productivity. Given the imperfect proxies for productivity and the inability to measure disclosure, then, the GSS sexual orientation variable will pick up the effect of both direct and indirect discrimination against groups A and B. If indirect discrimination lowers earnings less than direct discrimination, or if the net effect of nondisclosure is to induce gay employees to work harder and to *increase* productivity, then the sexual orientation coefficient will *underestimate* the negative effect of direct discrimination on earnings.

The impact of imperfectly measuring the sexual orientation variable can be analyzed similarly. Individuals in group D are not coded by the GSS variable as being gay. As with group B, they are unlikely to face direct discrimination but might have lower incomes from indirect discrimination. Workers in group C are likely to face discrimination but cannot be identified with the GSS data. Overall, though, the effect of misclassifying group C and D workers is likely to be negligible. First, if, as seems reasonable, the economic and social risk from disclosing sexual behavior to a survey interviewer is less than the risk from workplace disclosure, the combination of survey nondisclosure and workplace disclosure will be rare and the size of group C will be small. Second, unless the overall group of lesbian, gay, and bisexual adults is quite large, the addition of the potentially lower-income group C and D workers to the heterosexual total will have a relatively small effect on the average income against which behaviorally gay respondents' income is mea-

sured. And even a large combined group of C and D workers facing both indirect and direct discrimination would pull the average down, *reducing* the observed income difference between gay and heterosexual workers. Thus the errors both in measuring behavior and in using behavior as a proxy for disclosure result in a bias *against* finding discrimination.

As discussed above, workplace disclosure may be systematically related to income, and Schneider found that disclosure is less likely as income increases, resulting in another form of selection bias. This pattern is consistent with the hypothesis that gay workers make cost-benefit calculations before disclosing their sexual orientation, and that disclosure is less likely among high-income workers because they have more to lose. The earnings loss for lower-income workers who disclose their sexual orientation and face direct discrimination will not, therefore, be representative of the loss faced by the average worker. But in that case the average worker has a *higher* expected loss of income, which is why he or she does not disclose, suggesting that, again, this source of selection bias will result in underestimates of the average effect of disclosure and discrimination.

Aside from the effect of indirect discrimination on the productivity of gay workers who do not disclose their sexual orientation, other sources of ability or productivity differences that are unobservable to the researcher might be related to sexual orientation. For instance, some economists measuring racial earnings differentials have hypothesized racial differences in unobserved quality (usually differences in schooling quality) that, if properly accounted for, would reduce the difference in earnings attributed purely to racial discrimination (for example, Smith and Welch 1989; Juhn,

Murphy, and Pierce 1991). This is less likely to be an issue in the case of sexual orientation, however, since observed levels of education are quite similar across heterosexual and homosexual groups and there is no reason to expect systematic differences in the quality of schools attended by lesbian, gay, or bisexual people. Furthermore, much of the gay and lesbian identity development process occurs in the late teens and early twenties, suggesting that sexual orientation is unlikely to influence educational decisions (Garnets and Kimmel 1991).¹²

Other components of unobserved ability—work experience and labor force attachment—also cause concern when OLS models are used to measure discrimination against women (Bloom and Killingsworth 1982), and these factors, unlike the school quality component, may be correlated with sexual orientation. Lesbians and bisexual women may have stronger labor force attachment and more work experience than heterosexual women, since they usually have weaker economic incentives to specialize in home production: their partners (or spousal equivalents) are women who face the same potentially discriminatory labor market. Furthermore, the legal benefits of partnership are fewer, and the economic penalties for not working are greater even for lesbians who share economic resources and expenses with a partner, since few employers who provide health care benefits for employees' legal spouses do so for employees' same-sex domestic partners (Bowman and Cornish 1992). As a result, lesbians' labor force experience might not be interrupted as often as heterosexual women's, which would make the potential experience variable available from the GSS a closer proxy for lesbians' actual experience (see note 11).

If these unobserved components of wage determination are correlated with sexual

orientation, then OLS coefficients are biased *against* finding discrimination, since the unobserved components mean that lesbian and bisexual women are more productive than heterosexual women who are identical in their observed characteristics. This problem can be at least partly corrected with information gained from modeling the female worker's decision to work full-time. To the extent that the unobservable component involved in the full-time work decision is correlated with the unobservable determinants of wages (for instance, actual experience and labor force attachment), adding a Heckman correction term—the inverse Mills ratio representing the probability of being a full-time worker—to the OLS model will reduce the bias (Bloom and Killingsworth 1982). In addition, the possibility that the potential experience variable more closely proxies lesbians' actual experience than heterosexual women's actual experience will be accounted for by interacting experience and sexual orientation.

Results

Table 2 presents the means for the variables used in both procedures. Unlike the data derived from the surveys described in Table 1, the behaviorally lesbian/bisexual women earn approximately 18% less, on average, than do behaviorally heterosexual women in the GSS, and gay/bisexual men earn 7% less than heterosexual men. Two likely sources of the differences between the survey results in Tables 1 and 2 are differences in sampling techniques (the GSS is a probability sample, the other surveys are not) and in sexual orientation definitions (the GSS definition is based on behavior, the others on self-identity). Within the GSS, some of the unadjusted difference between lesbian/bisexual women and heterosexual women could come from the fact that the gay sample is slightly younger and less likely to be white, factors that would tend to reduce average incomes. For men, there are no obvious factors pulling down gay/bisexual men's earnings. The major difference seems to

¹²Although adolescents in the early stages of gay identity development might have painful high school experiences, the effect of this internal conflict on educational attainment is not clear (Gonsiorek 1988:474–77).

Table 2. Variable Means for Full-Time Workers, Pooled 1989–91 Data.
(Standard Deviations in Parentheses)

Variable	Lesbian/ Bisexual Women	Heterosexual Women	Gay/ Bisexual Men	Heterosexual Men
Annual Earnings (in 1988–90)	15056 (8284)	18341 (11334)	26321 (16937)	28312 (16842)
% 0 to 9,999	29.4	21.2	10.6	8.8
% 10 to 19,999	35.3	36.2	29.8	22.5
% 20 to 29,999	29.4	25.8	21.3	26.2
% 30 to 39,999	5.9	12.0	17.0	19.4
% 40,000 and up	0.0	4.7	21.3	23.1
Education (in years)	13.6 (3.0)	13.6 (2.5)	13.6 (4.0)	13.6 (2.9)
Age	34.0 (10.3)	39.4 (11.1)	41.3 (12.2)	39.1 (11.8)
Potential Experience ^a	15.4 (10.8)	20.8 (11.6)	22.6 (13.2)	20.4 (12.1)
% White	79.4	85.2	89.4	90.7
% Married	23.5	51.0	40.4	67.7
% in Large SMSA	55.9	51.9	55.3	45.8
Region (%):				
Northeast	17.6	20.2	25.5	18.4
Midwest	23.5	26.9	29.8	26.6
West	17.6	19.3	12.8	19.6
South	41.2	33.5	31.9	35.3
Occup. (%):				
Manager	8.8	14.8	12.8	17.9
Prof./Tech.	26.5	25.4	31.9	20.0
Clerical/Sales	14.7	35.7	17.0	12.9
Craft/Operative	23.5	12.3	27.7	40.6
Service	26.5	11.9	10.6	8.7
N	34	698	47	901

^aPotential experience = age – years of education – 5.

Note: The lesbian/gay/bisexual sample is made up of those with one or more same-sex sexual partners.

Source: Author's calculations from General Social Survey (Davis and Smith 1991).

be in the occupational distribution, but this difference would not necessarily mean lower incomes: the gay male respondents are less likely to be in managerial and blue-collar occupations but are more likely to be in professional/technical and service occupations.

Table 3 presents the OLS coefficient estimates from regressions on the subsample of the 1680 respondents who had full-time jobs when the survey was taken.¹³ Columns (1)–(3) and (4)–(5) present the results for women and men, respectively. Columns

(2), (3), and (5) supplement the basic specification in (1) and (4) with dummies for broad occupational category. (The small size of the gay sample prevented more detailed categories.) Table 3 reports the OLS results using only the most stringent definition of lesbian/gay/bisexual (having had at least as many same-sex as opposite-sex sexual partners). Using the three alternative definitions changes the L/G/B coefficient (as noted below), but the other coefficients vary only slightly. In all specifications, for both men and women, the education, occupation, marriage, race, sex, and experience coefficients have the usual signs, and most of them are significant at the 5% level.

¹³All calculations in this paper were derived using LIMDEP.

Table 3. Determinants of Annual Income in General Social Survey: Income in 1988–90.
(Regression Coefficients; Absolute Value of t-Statistics in Parentheses)

Variable	(1) Women	(2) Women	(3) Women	(4) Men	(5) Men
Constant	7.17** (37.1)	7.12** (34.6)	5.5** (4.1)	7.84** (60.0)	7.96** (51.6)
L/G/B ^a	-0.35 (1.1)	-0.32 (1.1)	-0.12 (0.3)	-0.28** (2.3)	-0.31** (2.6)
Education	0.13** (11.1)	0.10** (7.6)	0.13** (4.1)	0.09** (12.5)	0.07** (8.5)
Currently Married	-0.08 (1.4)	-0.08 (1.5)	-0.26 (1.6)	0.27** (5.8)	0.26** (5.6)
White	0.11 (1.4)	0.10 (1.3)	0.25 (1.5)	0.09 (1.2)	0.06 (0.9)
Potential Experience (Exper) ²	0.04** (5.1)	0.04** (5.1)	0.10** (2.1)	0.04** (6.2)	0.04** (6.1)
	-0.00** (3.9)	-0.00** (4.0)	-0.00* (1.8)	-0.00** (4.3)	-0.00** (4.3)
Exp*L/G/B	0.01 (0.8)	0.02 (1.0)	0.01 (0.3)		
Big SMSA	0.26** (4.5)	0.28** (5.0)	0.35** (3.5)	0.19** (4.4)	0.17** (4.1)
Northeast	0.07 (0.9)	0.06 (0.8)	-0.08 (0.5)	0.17** (2.9)	0.18** (3.6)
Midwest	-0.02 (0.2)	-0.01 (0.1)	-0.12 (0.9)	0.20** (3.8)	0.19** (3.6)
West	-0.05 (0.6)	-0.05 (0.6)	-0.17 (1.1)	0.09 (1.6)	0.08 (1.4)
Manager		0.58** (5.6)	0.57** (5.6)		0.35** (4.1)
Prof/Tech		0.58** (5.8)	0.57** (5.8)		0.31** (3.5)
Clerical/Sales		0.46** (5.3)	0.46** (5.4)		0.13 (1.5)
Craft/Operative		0.36** (3.4)	0.35** (3.4)		0.11 (1.4)
Lambda			0.90 (1.2)		
Adj. R ²	0.21	0.25	0.25	0.28	0.29
N	732	732	732	948	948

Note: Dummy variables for service occupation and South are excluded.

^aDefinition of L/G/B: Number of same-sex sexual partners \geq number of opposite-sex sexual partners

*Statistically significant at the 10% level; **at the 5% level (two-tailed tests).

When controls are included for the other factors influencing income, being behaviorally lesbian, gay, or bisexual reduces income, but the difference is statistically significant only for men. For women (column 1), the dummy variable for lesbian, gay, or bisexual is -0.35 using this L/G/B definition, and the coefficient ranges from -0.29

to -0.36 using the other three definitions (not reported in Table 1). The t-statistics are small, however, in each case. The null hypothesis that the coefficient is zero can be rejected in a one-tailed test at the 10% level for two of the four definitions, which define as lesbian or bisexual both those with more than one same-sex sexual part-

ner and those with either more than one or at least as many same-sex as opposite-sex sexual partners (definitions 2 and 4 above).¹⁴

For men (column 4), the sexual orientation effect is stronger than for women: the coefficient is -0.28 and is significantly different from zero at the 1% level in a one-tailed test. This finding suggests that the income penalty for gay or bisexual men could be as much as 24.4%.¹⁵ The coefficient using the other three definitions ranges from -0.12 to -0.24 in the specifications without occupation dummies. Two of those coefficients are significantly different from zero at the 5% level in a one-tailed test, and the third (having one or more same-sex sexual partners, the most general one) is significant at the 11% level in a one-tailed test. In general, as the definitions become more stringent, the income effect becomes more negative, suggesting that having more than one same-sex sexual partner or mostly same-sex sexual partners may identify those gay/bisexual men who are more likely to disclose their sexual orientation at work.

Escoffier (1975) suggested that some gay people might choose occupations in which workplace disclosure of sexual orientation is least damaging. In this case, disclosure is a compensating differential for the gay worker. An alternative explanation that would account for the same observations is that gay workers are segregated or crowded into more tolerant occupations that have lower wages. To see if occupational choice or crowding can explain lower gay incomes, I added four occupational dummy variables to each specification (the dummy variable for service occupations was excluded). The

effect seen in columns (2) and (5) was similar for all L/G/B definitions.

Adding occupation dummies has a very different effect on men and women. For women, both the coefficients and t-statistics for the sexual orientation variable drop considerably, and the occupation dummies have large, significant effects on income. In other words, accounting for the differences in occupational distribution explains some of the sexual orientation income difference (which also becomes statistically insignificant in all of the specifications including occupation). Lesbian/bisexual women tend to be in lower-paying occupations, as the means in Table 2 suggest: half of the lesbian/bisexual women work in the lowest-paying occupations for women in this study (craft/operative and service). After the selection bias correction in column (3), both the coefficient and the t-statistic are insignificantly different from zero.¹⁶ In a similar specification without occupation controls, the L/G/B coefficient was -0.11 with a t-statistic of 0.2. (In the selection-corrected specifications using the other definitions, the coefficient on L/G/B was sometimes positive and sometimes negative but never significantly different from zero.)

For men, adding the occupation variables *increases* the negative effect, revealing as much as a 26.7% income disadvantage (from column 5). Although occupational sorting might be observed at a finer level of detail not possible in this study, these results suggest that gay/bisexual men are in higher-paying occupations but earn less than heterosexual men within these broad categories.

Why does the effect of sexual orientation vary between men and women? The results for women from the selection-corrected specifications are sensitive to the sexual

¹⁴Also, the degree of statistical significance achieved depends on the specification of the model. Although the interaction of sexual orientation and experience is included for economic reasons, it should be noted that without that variable, the L/G/B coefficient is half as large and is statistically insignificant for all four definitions.

¹⁵The percentage decline is calculated using $\delta = \ln(1 + d)$, where δ is the coefficient on LGB and d is the percent difference in mean incomes between the gay and heterosexual groups.

¹⁶The selection correction involved first modeling the probability of women holding full-time jobs. The independent variables in this probit model included all the variables in Table 3 except for occupation. The coefficient on L/G/B was always positive, regardless of the definition, but never statistically significant.

orientation definition used. With actual measures of these unobserved characteristics, the sexual orientation income gap might widen for women. Also, behavior may not be a good proxy for identity as lesbians, reducing the expected income difference if identity is more stigmatizing than behavior, as seems likely. The variable means in Table 2 demonstrate that lesbian/bisexual women are not better off economically than gay/bisexual men, suggesting that gender is more important than sexual orientation in determining income for lesbian and bisexual women. On average, lesbian/bisexual women earn 57.2% of a gay/bisexual man's income; the female-to-male ratio among heterosexuals is 64.8%.

Another possible explanation is that gay/bisexual men face greater discrimination than lesbian/bisexual women for other reasons. Bloom and Glied (1988) suggested that employers might use perceived sexual orientation as a proxy for susceptibility to HIV and AIDS, since the Americans with Disabilities Act prohibits employment discrimination because of HIV status. This might account for some of the difference, since HIV is much rarer among lesbians.

Finally, the form of discrimination against lesbians may differ from that against gay and bisexual men. The results in Table 3 suggest that at least some of lesbian/bisexual women's earnings disadvantage comes from being in lower-paying occupations, perhaps as a result of discrimination. Indirect discrimination might affect women differently from men, since women already face the potential for sex discrimination and might not fear sexual orientation discrimination as much as gay men do. Or double jeopardy might encourage lesbians to work harder to avoid discrimination. In other words, the coefficients in Table 3 will then *underestimate* the impact of direct discrimination on earnings.

Conclusion

The findings of this study provide evidence that economic differences exist between people with differing sexual orienta-

tions (as defined by their behavior). Behaviorally gay/bisexual men earn from 11% to 27% less than behaviorally heterosexual men. Because this economic disadvantage holds after controlling for education and occupation, it appears that equally productive gay people are being treated differently, that is, they are being discriminated against. Although the findings for lesbians are not consistently statistically significant, the behaviorally lesbian/bisexual women in this sample earn less than similar behaviorally heterosexual women. The difference for lesbians ranges from 12% to 30%, dropping greatly in size and significance when occupation and a selection bias correction are taken into account. The lack of statistical significance could reflect the small size of the sample of lesbians or could result because the model does not adequately control for unobservable differences between lesbians and heterosexual women in labor force experience and work attachment.

Might this discrimination wither away on its own, as suggested by a taste discrimination model? An employer with a less intense taste for discrimination ought to be able to hire productive gay employees for a smaller outlay than other employers must pay for equally productive heterosexual workers, eventually bidding up the wage of gay employees. Many firms have added sexual orientation to their nondiscrimination policies, as the model predicts.

At least two factors suggest, however, that firms with nondiscrimination policies have not been responding simply to competitive pressures. First, anecdotal evidence suggests that some of these firms changed their official policies in response to state or local antidiscrimination ordinances (Badgett 1990:207). Others changed their policies at least partly in response to pressure and actions by individual gay employees and company groups (*Gay/Lesbian/Bisexual Corporate Letter* 1992), and groups of customers and investors have also attempted to influence corporate policies. Second, in some cases, labor costs might actually be higher for those companies than for more intense discriminators if antidiscrimination

policies lead to changes in the company's benefits programs, such as the introduction of benefits for the domestic partners of gay employees. Therefore, it is hard to see this trend as resulting from the process generated within the Becker discrimination model.

If, as policy decisions over the past three

decades suggest, the United States is moving toward a general policy of prohibiting employment discrimination on the basis of nonproductive characteristics, then this paper's finding that sexual orientation discrimination exists in the work force identifies another policy need: adding sexual orientation to antidiscrimination laws.

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